In connection with the above brief statement I offer the following remarks:—

1. The ranges of studies in the two colleges, the Academic and Scientific, are so diverse in character, that the interests of the students and of education are better subserved by two distinct faculties working separately, than by one single combined faculty. There is not in the Yale scheme that multiplicity of optionals before the students after they have entered the University, which inconveniently subdivides classes, offers inducements to indolence, and tends to break down thorough discipline and study; for, in the act of entering, the student decides as to the range of his optionals; and if afterwards not satisfied (which would seldom be the case) he can join the other college.

2. It might be supposed that the scheme would require an unnecessary duplication of professors. But this is not so at Yale. In the Academic College there are already four instructors in Greek, four in Latin, five in mathematics, physics, and astronomy; and the professors of rhetoric, history, moral and intellectual philosophy, &c., are more than well occupied with their academic labours. The scientific students, if embraced in the Academic College, would actually require as many additional instructors as are needed under the existing system of the University.

3. In some scientific departments in the Academic College (zoology and botany, for example), in which the instruction occupies but a small part of the college course, there is no objection to employing the services of some of the scientific faculty, if this is feasible; and, where possible, the academic faculty may serve the Scientific College. Moreover, while all lecture-rooms had better be separate, the more costly kinds of apparatus may well be used in common, in order to avoid needless expenditure.

4. It may be added that many scientific students commence their training as scholars by first graduating in the Academic College. For the higher training in science, such a preparatory course in the classics is believed to be eminently desirable. They then enter an advanced class in some one of the departments in the Scientific College, and take the degree of Bachelor of Philosophy, or of Civil Engineer; or by special proficiency, after two years of study, that of Doctor of Philosophy. The Scientific College also admits of partial courses of study which do not lead to any degree.

5. The modification in American colleges which is demanded by the vast development of the sciences of nature within the past century—the era of origination for many of them—and also by the contemporary progress of linguistic and other sciences, is accomplished by the Yale scheme through a method which does not sacrifice, in any degree, classical education, and which at the same time combines thorough literary culture with the widest range and highest development of scientific education. The Classical College stands beside the Scientific, open to all who desire to commence with a classical basis; and the Scientific College offers a thorough and liberal education for all who would pursue a more distinctively scientific course.

6. The Yale scheme contemplates no important change in the Classical or Academic College except in the elevation of the department of modern languages and literature; and its ideal with regard to modern languages cannot be wholly realised until a knowledge of French and German is given (like that of Latin and Greek) in preparatory schools, and required for admission to the college.

7. The great change that has taken place at Yale is in the introduction of its School of Science. This school is not the result of any preconcerted plan on the part of the University. It is a gradual growth of the past twenty years, urged on by the demand in the land for scientific knowledge among lovers of science, those seeking to become its teachers, and others interested in its practical departments; and it has been carried forward to its present organisation mainly through the labours and judgment of the scientific men who have been slowly gathered into its faculty. More than two-thirds of its endowments are due to private munificence, and the remainder to the National Agricultural and Mechanical Fund.

WALLACE ON NATURAL SELECTION

Contributions to the Theory of Natural Selection. A Series of Essays. By Alfred Russel Wallace. (London: Macmillan and Co., 1870.)

IN the discussions of the French Academy, to which we referred in a recent number, M. Elie de Beaumont ventured to describe Mr. Darwin's theory as *La Science Mousseuse.* The phrase is a good one, and expresses very happily the kind of work for which some of the speakers in that debate are distinguished. But although we too in England are not unacquainted with this kind of popular science, scientific works do from time to time appear which are popular without being frothy, and to this class the present book belongs. While strictly accurate in matter, it is easy in style, and is so free from technical language, that it may be understood by educated men who are not professed naturalists; so that we hope it will be read by a large number of those to whom Mr. Wallace's delightful volumes have made the Malay Archipelago familiar.

The arrangement of the essays (most of which have been published separately) does not, perhaps, bring out their mutual connection so well as might be, and there is no attempt to blend them into a continuous series. Four main subjects are discussed, and each has its own peculiar interest.

The first and second chapters are reprinted as originally written in the East Indies, and, with the eighth, form Mr. Wallace's contribution to the theory of natural selection in general. It is remarkable that the same pregnant idea which Mr. Darwin has for ever united with his name should have occurred independently to another English naturalist on the other side of the globe. The public opinion of the scientific world will no doubt assign Mr. Wallace the full credit which the preface to this volume so modestly claims; and the highest respect is due to his varied and fruitful labours in both hemispheres; but a warmer feeling than respect will be paid to the spirit by which the following passage was prompted:—"I have felt all my life, and I still feel, the most sincere satisfaction that Mr. Darwin had been at work long before me, and that it was not left for me to attempt to write 'The

Origin of Species.' I have long ago measured my own strength, and know well that it would be unequal to the task. Far abler men may confess that they have not that untiring patience in accumulating, and that wonderful skill in using, large masses of facts of the most varied kind—that wide and accurate physiological knowledge, that acuteness in devising and skill in carrying out experiments, and that admirable style of composition, at once clear, persuasive, and judicial—qualities which in their harmonious combination mark out Mr. Darwin as the man, perhaps, of all men now living, best fitted for the great work he has undertaken and accomplished."

The third chapter is on so-called Mimicry among Animals, and contains an account of some of the remarkable cases of dimorphism observed by the author and by Mr. Bates, and of those in which one species closely resembles not only leaves and inanimate objects, but other specially protected animal forms. The facts thus established are explained with great ingenuity, and often with equal probability, by the operation of the natural laws of selection. The ways in which even brilliant colouring may become a means of protection are well illustrated, so that this branch of study is made to yield support instead of difficulty to the Darwinian theory. The following chapter, the only technical one in the book, is an application of the same law to explain the various forms and distribution of the group of *Papilionidæ*. It may be compared with Fritz Müller's study of the Crustacea from a similar point of view; and we believe that more solid progress will be made by carefully working out the application of natural selection to restricted and well-known animal groups than by attempting the construction of more comprehensive and imposing phylogenies.

In the seventh chapter Mr. Wallace makes a somewhat similar inquiry into the relation of the colour of birds to the form of their nests, and concludes, from a very wide survey, that when the female is of conspicuous colours, the nest is adapted to conceal her during incubation, while open nests are made by those already sufficiently protected. The exceptions to the rule are candidly stated, and most of them satisfactorily met. That the true law of the habit has been discovered is, perhaps, too much to say; but the evidence is at least enough to lead to further investigation on this interesting subject. Another essay, styled, not very happily, "The Philosophy of Birds' Nests," attempts to explain the building of nests and also the song of birds as the result not of "instinct," but of conscious imitation; gradual improvement being of course brought forward by the survival of the most skilful architects and the constant sexual demand for the best musicians. But not only does Mr. Wallace thus raise nest-making to the rank of an intelligent art, he also shows how much of human construction is simply imitative, and therefore as fairly to be called instinctive as a bird's; while in another passage he shows how the alleged wonderful displays of instinct in savages are really the result of habit and of reason.

This chapter on Instinct in Men and Animals would naturally introduce the last two, in which the working and the limits of the law of natural selection on the human race are considered. This is probably the most difficult, as it is certainly the most generally interesting, of the questions affecting the origin of animal species. Those who are not satisfied with the genealogies of Haeckel, and wait for the more cautious and philosophic conclusions expected from the master of the subject, will scarcely, we think, accept the views propounded in this volume by Mr. Wallace. He points out very clearly how most of the human peculiarities of structure may be supposed to have originated by the survival of the forms fittest for their mode of life, and is fully aware of the necessary change going on at the same time in the various functions, to bring them also into harmony with structure. And he shows with great justice how mental and moral qualities must interfere with the absolute carrying out of the law of natural selection—not only in civilised communities, where it is continually and designedly contravened, but among all savages who take, for instance, the least care of the sick and aged of their tribe. But, beside and apart from the operation of the general law of organic life, with these various modifications and restrictions, Mr. Wallace believes that another and independent cause has been at work in the evolution of the human frame, and that this has been a supernatural one. He maintains that the large size of the brain in man, the scantiness of his hairy covering, the great specialisation of his extremities, and some other peculiarly human characters, cannot be explained, except as the result of the direct action of the Creator's will. In fact, he compares man as he at present exists with such products of artificial human selection as the seedless banana or the London dray-horse; so that, if we may thus express Mr. Wallace's theory, man is God's domestic animal.

A great deal of the metaphysical discussion which occupies the last pages of the volume, including the verses quoted from an American poetess, has, we confess, to our mind, the same "double disadvantage" which the author finds in "the law of unconscious intelligence pervading all organic nature put forth by Dr. Laycock, and adopted by Mr. Murphy," that, namely, of being "both unintelligible and incapable of proof;" but the theory of divine artificial selection supplying the deficiencies of natural selection in the formation of man may, we think, be at once met by the following considerations.

The theory of natural selection does not suppose a kind of large female divinity, whose name is Nature, and whose function is to select from animals and plants those fittest for survival. The theory rests, as Mr. Wallace, in another part of his work, is careful to remind the reader, on certain proved facts (enumerated at p. 302), which necessitate the survival of certain forms by virtue of the proved physical laws which we see in daily operation. But these so called laws are, to all who believe in a Creator, simply the manner of His action. To say that our brains were made by God, and our lungs by natural selection, is really to exclude the Creator from half His creation, and natural science from half of nature. All the phenomena we know are of necessity ultimately referable to the First Great Cause: the object of science is to discover their secondary causes; and if the theory of natural selection does not explain how the larynx or the brain of man were developed, then we must try to find another which will. To fall back for explanation upon the primary efficient cause of their existence and the design with which they were framed, is only to confuse two distinct branches of inquiry.

At present, however, we may be content to see how far we can work the Darwinian hypothesis, and can only hope that there may be other "contributions" to the theory as interesting and valuable as these of Mr. Wallace.

P. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Dr. Bastian and Spontaneous Generation

I FIND that the "Address" which it was my duty to deliver at Liverpool, fills thirteen columns of NATURE. The "Reply" with which Dr. Bastian has favoured you occupies fifteen columns, and yet professes to deal with only the first portion of the "Address." Between us, therefore, I should imagine that both you and your readers must have had enough of the subject; and, so far as my own feeling is concerned, I should be disposed to leave both Dr. Bastian and his reply to the benign and Lethean influences of Time.

But I am credibly informed that there are persons upon whom Dr. Bastian's really wonderful effluence of words weighs as much as if it were charged with solid statements and accurate reasonings; and I am further told that it is my duty to the public to state why such distinguished special pleading makes not the least impression on my mind. With your permission, therefore, I will do so in the briefest possible manner.

The first half of Dr. Bastian's "Reply" occupies seven columns of your number for the 22nd of September. In all this wilderness of words there is but one paragraph which appears to me to be worth serious notice. It is this:—

"In the first place, he does not attempt to deny—he does not even allude to the fact—that *living things may and do arise as minutest visible specks, in solutions in which, but a few hours before, no such specks were to be seen.* And this is in itself a very remarkable omission. The statement must be true or false—and if true, as I and others affirm, the question which Professor Huxley has set himself to discuss is no longer one of such a simple nature as he represents it to be. It is henceforth settled that as far as *visible* germs are concerned, living beings can come into being without them."

If I did not allude to the assertion which Dr. Bastian has put in italics—it is because it bears absurdity written upon its face to any one who has seriously considered the conditions of microscopic observation. I have tried over and over again to obtain a drop of a solution which should be optically pure, or absolutely free from distinguishable solid particles, when viewed under a power of 1,200 diameters in the ordinary way. I have never succeeded; and, considering the conditions of observation, I never expect to succeed. And though I hesitate to speak with the air of confident authority which sits so well on Dr. Bastian, I venture to doubt whether he ever has prepared, or ever will prepare, a solution, in a drop of which no "minutest visible specks" are to be seen by a careful searcher. Suppose that the drop, reduced to a thin film by the cover-glass, occupies an area $\frac{1}{3}$ of an inch in diameter; to search this area with a microscope in such a way as to make sure that it does not contain a germ $\frac{1}{40000}$ of an inch in diameter, is comparable to the endeavour to ascertain with the unassisted eye whether the water of a pond, a hundred feet in diameter is or is not absolutely free from a particle of duckweed. But if it is impossible to be sure that there is no germ $\frac{1}{40000}$ of an inch in diameter in a given fluid, what becomes of the proposition so valuable to Dr. Bastian that he has made your printer waste special type upon it?

I now pass to the second part of the "Reply," which, though longer than the first, is really more condensed, inasmuch as it contains two important statements instead of only one.

The first is, that Dr. Bastian has found *Bacterium* and *Leptothrix* in some specimens of preserved meats. I should have been very much surprised if he had not. If Dr. Bastian will boil some hay for an hour or so, and then examine the decoction, he will find it to be full of *Bacteria* in active motion. But the motion is a modification of the well-known Brownian movement, and has not the slightest resemblance to the very rapid motion of translation of active living *Bacteria*. The *Bacteria* are just as dead as those which Dr. Bastian has seen in the preserved meats and vegetables; and which were, I doubt not, as much put in with the meat, as they are with the hay, in the experiment to which I invite his attention.

The second important statement in the second part of the "Reply" is:—

"Professor Huxley is inclined to believe that there has been some error about the experiments recorded by myself and others."

In this I cordially concur. But I do not know why Dr. Bastian should have expressed this my conviction so tenderly and gently as regards his own experiments; inasmuch as I thought it my duty to let him know both orally and by letter, in the plainest terms, six months ago, not only that I conceived him to be altogether in the wrong, but why I thought so.

Any time these six months Dr. Bastian has known perfectly well that I believe that the organisms which he has got out of his tubes are exactly those which he has put into them; that I believe that he has used impure materials, and that what he imagines to have been the gradual development of life and organisation in his solutions, is the very simple result of the settling together of the solid impurities, which he was not sufficiently careful to see, in their scattered condition when the solutions were made.

Any time these six months Dr. Bastian has known why I hold this opinion. He will recollect that he wrote to me asking permission to bring for my examination certain preparations of organic structures, which he declared he had clear and positive evidence to prove to have been developed in his closed and digested tubes. Dr. Bastian will remember that when the first of these wonderful specimens was put under my microscope, I told him at once that it was nothing but a fragment of the leaf of the common Bog Moss (*Sphagnum*); he will recollect that I had to fetch Schacht's book "Die Pflanzenzelle," and show him a figure which fitted very well what we had under the microscope, before I could get him to listen to my suggestion; and that only actual comparison with *Sphagnum*, after he had left my house, forced him to admit the astounding blunder which he had made.

To any person of critical mind, versed in the preliminary studies necessary for dealing with the difficult problem which Dr. Bastian has rashly approached—the appearance of a scarlet geranium, or of a snuff-box, would have appeared to be hardly more startling than this fragment of a leaf, which no one even moderately instructed in vegetable histology could possibly have mistaken for anything but what it was; but to Dr. Bastian, agape with speculative expectation, this miracle was no wonder whatever. Nor does Dr. Bastian's chemical criticality seem to be of a more susceptible kind. He sees no difficulty in the appearance of living things in potash-alum, until Dr. Sharpey puts the not unimportant question, whence did they get their nitrogen? And then it occurs to him to have the alum analysed and he finds ammonia in it.*

And as to the elementary principles of physics—in his last communication to you, Dr. Bastian shows, that he is of opinion that water in a vessel with a hole in it, from which the steam freely issues, may be kept at a temperature of "230° to 235° F. for more than an hour and a half."† I hope that Professor Tyndall, whom Dr. Bastian scolds as authoritatively and as unsparingly as he does me, will take note of this revolutionary thermotic discovery, in the next edition of his work on Heat.

It is no fault of mine if I am compelled to write thus of Dr. Bastian's labours. I have been blamed by some of my friends for remaining silent as long as I have done concerning them. But when, because I have preserved a silence, which was the best kindness I could show to Dr. Bastian, he presumes to accuse me publicly of unfairness, and to tell your readers that my Address "is calculated to mislead" them, I have no alternative left but to give them the means of judging of the competency of my assailant.

Jermyn Street, Oct. 10 T. H. HUXLEY

Ozone developed by Humidity and Electricity

IN confirmation of the idea as to the connection of excess of ozone with humidity and electrical action, to which attention was drawn in my previous letter of August 4, I forward you the following observations.

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* See NATURE, No. 36, p. 198.

† Ibid, No. 48, p. 433.